

## MD INTERVIEW

# AN INTERVIEW WITH WILLIAM A. BROCK

*Interviewed by Michael Woodford  
Princeton University*

October 22 and December 11, 1998

William A. Brock has taught in the Departments of Economics at the University of Rochester, Cornell University, and the University of Chicago, before moving to the University of Wisconsin, Madison, where he is now Vilas Research Professor of Economics. He has also been an External Professor at the Santa Fe Institute since 1989. His many awards include election as a Fellow of the Econometric Society in 1974, a Fellow of the American Academy of Arts and Sciences in 1992, and as a Member of the National Academy of Sciences in 1998. His over 100 invited lectures include presentations in Poland, Belgium, France, Mexico, Canada, Japan, Australia, England, Sweden, Norway, and Australia.

Brock has been a leading contributor to the development of methods of intertemporal equilibrium analysis in the areas of growth theory, monetary theory, and finance. More recently, he has played a crucial role in the development of new methods for the analysis of nonlinear dynamics in macroeconomics and finance, and models of expectations that relax the assumption of rational expectations.

I interviewed Brock in the faculty cafeteria near the Social Science Research Institute building, on the Madison campus of the University of Wisconsin, on October 22, 1998. Our interview was interrupted when he had to go to teach, and then was finished by phone on December 11, 1998. The transcript that follows does little justice to the liveliness of Brock's conversation, mainly because of my inability to transcribe the animated facial expressions and hand gestures that made the mathematical references more concrete. I have added several footnotes to the transcript, identifying some of the published sources that were mentioned during our conversation.

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**Woodford:** Let's start by talking about how you got into economics. That wasn't your first field of interest, was it?

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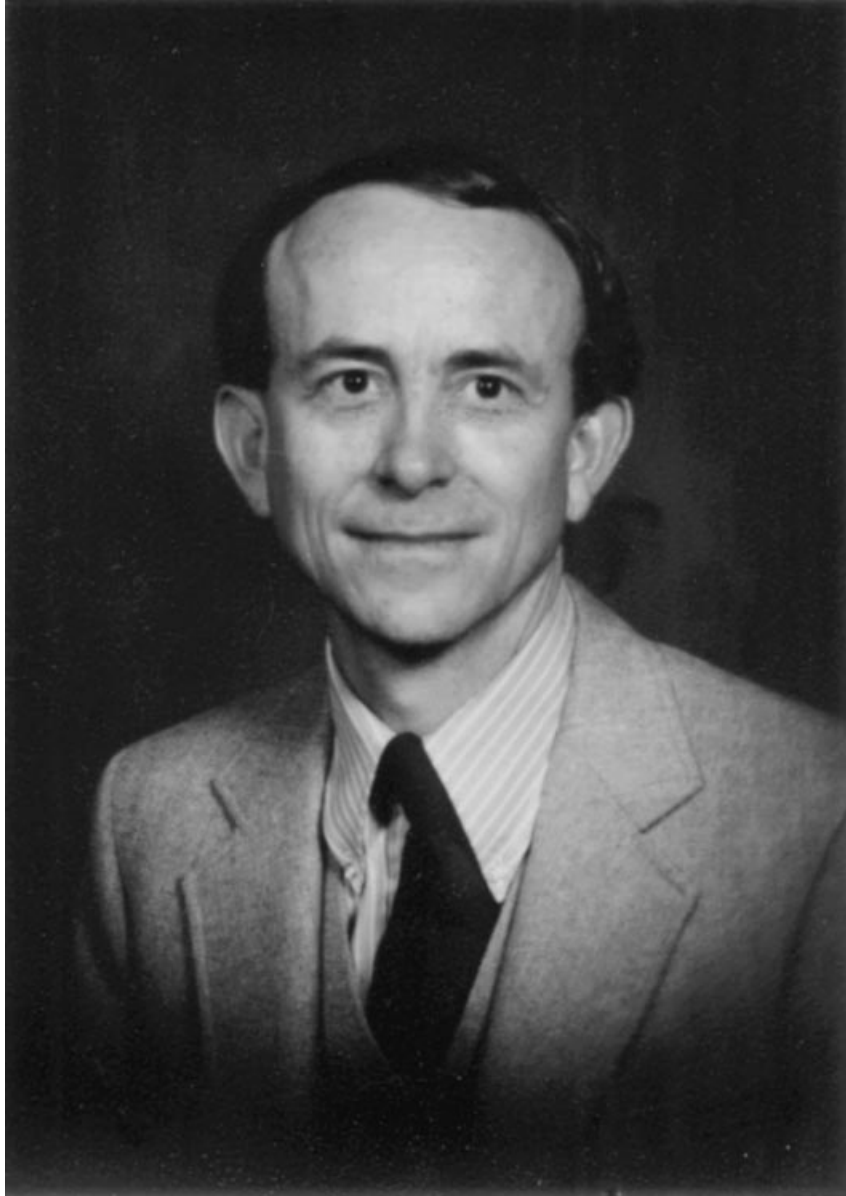


FIGURE 1. William A. Brock.

**Brock:** That's right, though I was always interested in economics. I grew up on a farm where I saw economics working first hand, you see; some people go bust, some people prosper, and some die. I also saw how little an effect an individual farmer had on the market, so you had to take all prices and everything under the

sun given. And so, I just became fascinated with the subject. Why is it that things just happen to us, some of which are mighty unpleasant? And it always seemed that when times were good, in the hog business for example, and when you got set up to raise hogs, the price would bust. So, understanding the dynamics of entry and exit fascinated me.

As an undergraduate I was actually in mathematics, but I worked for a team of economists lead by Russell Thompson of the University of Minnesota. He had come down to the University of Missouri at Columbia, and he hired me as a sophomore, I believe. I actually did some research with him as an undergraduate.

**Woodford:** What kind of a project was that?

**Brock:** We did everything under the sun. I remember one thing we had worked on was location theory. It was an early type of Loschian location theory. I had read a lot of that early literature. It's what we call "the new economic geography" nowadays.

**Woodford:** That's actually become a popular topic again quite recently.

**Brock:** Of course we were just fooling around with it, but I remember reading a lot of literature, about things like circular areas, and where you optimally locate milk plants when you had to compete with neighboring milk plants. Russell was very interested in that, you know.

**Woodford:** So you were sure from early on that economics was in fact what you were interested in working on?

**Brock:** Actually, yes. But I had asked Russell about going on to graduate school in economics, and he suggested that what I should do is get a Ph.D. in mathematics while I was young. That way I would have no mathematical hang-ups. Math would be second nature, and I could just concentrate on the economic substance of what I was doing. I would not have any mathematical inferiority complex. To this day I think it was the best advice that I have ever gotten from anybody. I am grateful for it. So, I never really had to think about mathematics, it sort of came naturally.

**Woodford:** Would you give the same advice to an undergraduate interested in economics today?

**Brock:** Well, I don't know. It was a long row to hoe. And I think things are more rigid now. At a lot of universities, I don't think they would hire a guy like me. I graduated with a math degree from the University of California-Berkeley—David Gale was my thesis adviser—and to put it quite bluntly, I didn't know any economics. It's one thing to work as a research assistant for an economist, and quite another thing to go through a formal Ph.D. program in economics. I really didn't know any economics, except to have a kind of instinct for it, but that's not the same as having formal training, and today I don't think anybody would hire me.

Nowadays, you know, things are too rigid. I know how many departments make appointments—looking at credentials, counting publications, rating journals, and all that kind of stuff.

**Woodford:** Do you think that we are getting the wrong people by applying these criteria?

**Brock:** I would urge more flexibility. Otherwise you can't take advantage of an opportunity. When Lionel McKenzie hired me at Rochester, he got interested in the thesis work that I was doing with David Gale, and so Lionel just hired me. The way it looked to me then, they just hired guys.

**Woodford:** McKenzie could just say, hire that guy.

**Brock:** [Laughs] Yes, I couldn't believe it. It wouldn't happen today, but maybe something like that might happen at Chicago. I can remember when another student of David Gale's was on the market, and Chicago was interested in him. He didn't have any economics background either. They were willing to consider him, and I think that we even made him an offer, though here my memory is poor. But I remember that there wasn't any issue about what his degree was in or whether he was appropriately certified.

**Woodford:** You were also unusually fortunate, as a math Ph.D. hoping to break into economics, to have been able to work with David Gale. There are probably not a lot of people in math departments that are as interested in economics as he was.

**Brock:** That's right. That was key. I would say that Russell Thompson for mentoring me as an undergraduate, David Gale for mentoring me as a graduate student, and Lionel McKenzie for mentoring me as a young pup were all key. If it weren't for those three guys, I wouldn't be an economist, probably. They were key.

**Woodford:** What was it like to join the faculty at the University of Chicago? At the time, you were probably the only person whose approach to economics was very mathematical.

**Brock:** You're right. That was pretty early. Those were the days of Milton Friedman and George Stigler, the grand old men of the profession.

**Woodford:** Did they feel that having a mathematician in the department, or someone with serious mathematical training, was useful to economics?

**Brock:** That was hard to figure out. I think it was part of the risk-taking nature of those guys; they like right-skewness. I got the impression that they thought it would be fun to have one character, that maybe the department could afford one real character.

**Woodford:** So they could at least appreciate the option value.

**Brock:** Yeah, they saw that there was some option value, even though the mean was negative, and the variance was extremely large. There was some mass in the right tail, but it was a risk. [Laughs.]

Yet I think they were supportive, in their own way. A lot of people wouldn't think so. I remember that some people in mathematical economics, my own field, thought that I was somewhat of a traitor, and strange to say the least, to go to a place like the University of Chicago. But I have always enjoyed taking a risk and doing the unusual. I enjoy it to this day. I even tried out hang gliding once.

**Woodford:** When was that?

**Brock:** That was when I was teaching at Cornell. Another fellow and I went out and took hang-gliding lessons. I actually did a few flights.

**Woodford:** How did your career develop from there?

**Brock:** Next I was at Rochester. I loved it at Rochester, but then when the opportunity came to go to Chicago, I just thought it was too good an opportunity to turn down, just to find out what it was like to be at a place like that. And they take economics so seriously, though of course Rochester did too. In fact, I don't think I have seen two departments anywhere, and I have been in a lot of departments, that have the degree of seriousness about science that Chicago and Rochester have.

**Woodford:** How did going to Chicago affect the development of your interests or your ideas?

**Brock:** Well, I got interested in monetary theory from having gone to Chicago. At Rochester, I was interested in turnpike theory, because Lionel [McKenzie] was there. At Chicago, I got to be fascinated by Milton Friedman's "optimum quantity of money." I thought that it would be interesting to formalize that in the context of a growth model. So I took the Sidrauski model as a building block, and formalized the notion of a perfect-foresight equilibrium, which nowadays would be called a rational point expectation, I suppose.

I got to be fascinated with trying to classify the number of equilibria you could have in a model like that. You essentially end up with something kind of like a fixed-point problem, taking a function space to a function space, which locates a fixed point that solves a differential equation. What was fascinating to me about that was the number of equilibria that turned up near the full satiety point.

But in those days you were penalized for reporting a large number of equilibria. The name of the game was to impose discipline on the system. You tried to get rid of equilibria, not to find as many as you can possibly manufacture. I can imagine that if a man from Mars came and looked at today's economics profession, he would think people were paid a piece rate for the number of equilibria they found. But in those days you had to pay a fine for each one that you found. So I struggled mightily to get rid of those things. Of course, I couldn't, so I reported them in the paper, and stuck equilibria into various footnotes where they wouldn't be quite so prominent, hoping that they wouldn't alert the notice of the referees. So I finally got the thing published even though it is filled with equilibria.

**Woodford:** Did the referees ask you to deemphasize those results?

**Brock:** Well, I had three referees, and they were almost orthogonal in what they wanted me to do. So I really felt like I was being pulled in a three-dimensional vector space, while I only had two dimensions or one dimension to work in, trying to figure out how to comply. So it was kind of frustrating, but it was published anyway.<sup>1</sup>

**Woodford:** You were involved a lot with optimal growth theory, back in the 1970s.

**Brock:** That's right. I got involved with that working with David Gale. David Gale had a paper with a rather lengthy proof of the existence of an optimal growth path under the overtaking criterion. I got to looking at that, and I figured out how to redefine the notion of overtaking so that it wasn't quite so difficult to work with mathematically. I called the notion "weakly maximal." Then one could just rearrange a bunch of terms involving support prices, and get the infinite series for

the Ramsey–Weizsacker overtaking ordering; this may or may not be absolutely convergent, which is a technical problem that causes a major headache trying to prove theorems in this area.

I figured out<sup>2</sup> how to rearrange those terms into the negative of a sum of positive terms plus a tail. And then if you had a sum of positive terms, you could minimize the sum of positive terms, and then do something like the Cantor diagonal process, to actually construct a minimizer to achieve the infimum of the sum of the positive terms. Fatou's lemma lets you exchange the order of the integral and the limit operations, but you didn't have that tool available unless the functions were nonnegative or could be transformed into units where they were. So, recognizing how to use that tool simplified that proof a lot. And I could control the tail term; in any candidate optimum you could not diverge much from the steady state, at least not for too much of the time, if the steady state were unique, because you would suffer too much loss of value. This was a classical argument that dated back to Radner, McKenzie, and Gale. And so I could use that to control the tail term. Then if I could just control the infinite series by the Fatou device, I could simplify the proof that was one of the major pieces of my thesis.

And then people started using that simplification, and various extensions were made to it. The Russians got interested in it; Arkin and Evstigneev produced a book on stochastic optimal growth theory in multisector economies,<sup>3</sup> and that device was used there. It made it a lot easier to prove stochastic analogs of turnpike theorems, and take care of existence at the same time.

**Woodford:** What in your view was the most important aim of work on optimal growth theory? Did you intend it as a theory of long-run economic growth primarily? Or were you simply interested in having a consistent way of thinking about dynamic issues, broadly speaking, like modeling saving behavior, and investment, and so on? Another view, probably that of the Russian school, would be that it was intended to contribute to the theory of economic planning. What was your point of view?

**Brock:** At first I think I simply had the posture of the mathematician, who was just interested in how to solve technical problems, but then I learned more about economics, after about a year or two at Rochester. I just sort of learned economics, not only by teaching but essentially by tormenting the faculty, asking people like Sherwin Rosen, and anybody else I could get my hands on, dozens of economics questions, until they got exhausted from constant questioning. Then I got fascinated with the idea that these models could be looked upon much like you do in welfare economics, where if markets bring about a Pareto optimum, there will be an as-if maximization problem, a certain sum of utilities that the equilibrium acts as if it is maximizing. I got interested in that kind of thing, and using the models as tools to study competitive equilibrium.

**Woodford:** So they could model how competitive equilibria unfold over time.

**Brock:** I wrote a little thing as a discussion of a paper by Roy Radner. I think this was at the Toronto Winter Meeting of the Econometric Society, in 1972.<sup>4</sup> It was early in the morning and the audience seemed kind of drowsy, so I decided to do

something crazy. And so I took the neoclassical stochastic growth model that I was working on with Mirman, and said, let's think of this as a competitive equilibrium for an economy. What would it look like? You would see random movements in capital and consumption, et cetera. And maybe that would look like something bad, but it's a competitive equilibrium, so it's Pareto optimal; you can't beat it.

And I didn't think that was exciting enough to wake up the audience, so I proceeded to show how Marx's labor theory of value breaks down in a world like this. I said, recall the nonsubstitution theorem, for multifactor setups with one primary factor of production, no joint production, and so on. In that equilibrium the relative price of goods would be the relative congealed labor contents. I showed that the analog of that in the deterministic growth model is that the capital-labor ratio in steady state depends only on the subjective rate of time discount on the future utility. It doesn't depend on any parameters of the period utility function.

But that's all for the case of certainty. In the stochastic growth model, parameters of the utility function all get wadded up into the stochastic steady state. So there is the end of the labor theory of value. And, I think I made some smart-aleck remark about Marx not having thought about that, and its striking a gaping hole in his theory, but I still don't think I was successful in waking up the audience at that time in the day.

I thought that was fascinating. There were a bunch of other people, too, of course—Lucas, Prescott, the rational expectations literature, and so on—but I was too naïve to understand what I was doing, and that these things were all related.

**Woodford:** Your model with Mirman<sup>5</sup> was subsequently invoked as providing the foundations for the kind of stochastic growth model used in real business-cycle theory. Was that the sort of application you had in mind at the time?

**Brock:** I hadn't really thought of that at the time. You know, I wish I had thought of that. I had thought of it in terms of decentralization of an optimal allocation—that you could manufacture a competitive equilibrium, kind of like in the classical papers of Debreu, where you maximize a weighted sum of utilities subject to a bunch of constraints, and then under the right kinds of assumptions, you could manufacture competitive equilibria corresponding to the optimization problems. I thought, well, you could do that in infinite-dimensional spaces, too; isn't that neat. But those guys were really clever in recognizing that you could actually do business-cycle theory using that kind of model as the base. Maybe Mirman might have thought of it, but I was still muddling around in pure mathematics.

**Woodford:** You have since then gotten very interested in nonlinear dynamics in economics.

**Brock:** Yes. I would like to think of that as a continuation of the theme of my research, although I'm sure a lot of outsiders don't look at it that way. Because my research started out by locating sufficient conditions for stability in heterogeneous capital goods models, and that problem still fascinates me to this day. The basic idea of that work was that, if you have enough market completeness, so that you can do enough smoothing across time and space, and if people don't



discount the future very much, then you can essentially adapt value-loss-type arguments to argue that the economy can't spend too much time away from the maximal steady-state point, or its stochastic analog. I was just fascinated with those kinds of technical problems, and also thought that they had some importance in understanding the forces that dampen economic instability. And then it was natural, having studied one side of the problem, to look at the flip side: what are the sources for instability?

I wrote a paper that was a lecture at the 1975 World Congress of the Econometric Society,<sup>6</sup> where I discussed the engineering literature on optimal linear regulator problems. I had taught a course on this when I first started teaching at Chicago, and showed students how you could borrow useful techniques from this literature in engineering. You have a set of differential equations that are like

$$dx/dt = Fx + Gu,$$

where  $u$  is your control, and you have payoffs like  $x'Qx + u'Ru$ , where  $Q$  and  $R$  are positive-definite matrices, and you want to minimize the undiscounted sum of payoffs. That's the engineer's problem. The economist maximizes the discounted sum of course, but you can make a change in the units and absorb the discount factor in the  $F$  matrix of the differential equation, and then directly apply the standard results.

So you can see exactly what the forces for stability are by looking at the engineering literature: low discounting, large enough eigenvalues of the  $Q$  and  $GR^{-1}G$  matrices, and enough inherent stability in your underlying dynamics defined by the  $F$  matrix. For example, if the  $F$  matrix is stable, then you've got to work to destabilize the system by applying costly control. So you can start to understand from these linear dynamics the forces that cause local instability. You can think of the engineering literature as a kind of a linear-quadratic approximation to a nonlinear system. And if instead of doing the usual thing of linearizing and looking at eigenvalues, you do a linear-quadratic approximation and then look at the resulting optimal linear regulator problem, you can tune your mind to find not only the forces for stability, but also the forces for instability.

So you start looking at the elements, and you start getting an understanding of what the forces of stability and instability are, what the economic trade-offs are. My work in the 1970's, with Scheinkman, Mirman, Majumdar, and Magill—I hope I'm not forgetting anyone, because I worked with so many people on this—was mostly focusing on the forces of stability, which was sort of considered the name of the game then. I find it a bit odd, but it was kind of understood in the 1970's that forces for instability were everywhere. The hard mathematical problem was finding sufficient conditions on utilities and the dynamics so that in the face of heterogeneous goods—because there were a lot of interactions, complementarities, substitutabilities, etc., that would lead to forces for instability—it was sort of thought in those days that the analytic and intellectual challenge was to find sufficient conditions for stability.





**FIGURE 2.** From left to right, Mukul Majumdar, Tapan Mitra, José Scheinkman, Lionel McKenzie, and Buz Brock, during summer 1975, at a conference on capital theory at Minary Conference Center, Dartmouth College, Squam Lake, New Hampshire.

**Woodford:** But when you say that it was understood that forces for instability were everywhere, do you think that it was believed that in practice the forces for instability were important? Or was it simply a challenge to get the mathematics to align with people's economic intuition, which was that the forces for instability were trivial in practice?

**Brock:** Yes, I think that in Chicago people thought the forces for stability were very strong, and that kind of fits in with the philosophy of the place. For a mathematical person like Lionel McKenzie, I think his focus on the challenging mathematics probably made him nervous about being too religious about making any arguments for the stability of capitalism. And then for guys like Arrow and Hahn, Quirk and Saposnick, and so on, in general equilibrium theory, well, they knew that it was a nightmare of instability—Scarf's example and all that. And those of us working in heterogeneous-capital-goods optimal growth theory felt that the same threat was there for us too. So the hard mathematical problem was to find sufficient conditions for stability, especially global stability.

**Woodford:** Because it was believed that finding counterexamples to the stability results would be easy.

**Brock:** That's right. That's the mathematician's attitude. When you're reared as a graduate student in mathematics, at least at Berkeley, the first thing that you're taught is how to find counterexamples. Like a function that's continuous everywhere but differentiable nowhere; that's the kind of stuff you cut your teeth

on, how to find examples of nonmeasurable sets. Gelbaum and Olmsted's book, *Counterexamples in Analysis*,<sup>7</sup> was popular with us grad students. So we all took it for granted that this is an easy game to play. What's hard is to find a robust set of sufficient conditions that are kind of surprising, that deliver a surprising conclusion.

**Woodford:** So you weren't surprised at all that later authors could demonstrate the possibility of nonconvergence to a turnpike in optimal growth models?

**Brock:** That would be the attitude of the mathematician, but not the attitude of an economist, because what the economist is after are substantive issues. But from the point of view of the mathematician, sort of flexing his mathematical muscles, one would not be so excited. The mathematician would be more excited with the theorem that just struck him as having a conclusion that was surprising, and with why a particular sufficient condition, through a rather long chain of reasoning, would lead to this conclusion.

**Woodford:** Do you have a view about how the literature has come out, in terms of whether we should view the turnpike theorems as telling us the economically important results about dynamic competitive systems, or whether the things that have been learned about conditions that can give rise to instability should bulk larger in people's view of what we understand about the dynamics of competitive economies?

**Brock:** I think what you learn out of the turnpike literature, and out of general equilibrium theory, and so on, though you probably couldn't prove this as a mathematical theorem, is that stability is pretty robust. I mean that you can find useful sufficient conditions for stability that make economic sense. You can prove it if you assume complete markets, and so on, though of course nobody believes that. But even with incomplete markets, there are arguments like those of Ehrlich and Becker, or Bewley's discussion of the permanent-income hypothesis, where you can sort of do homemade market completion. Intuitively, with more and more market completion, and longer-lived agents that could borrow and lend to get around liquidity constraints, like the stuff that you looked at, for example, well, then, that would argue for stability.

Of course, there are technical problems, like if one person discounts the future less than everybody else, and acts like a Saudi Arabian dynasty thinking ahead 900 years, rather than thinking ahead a short time as Americans are often characterized. You can show then that that guy will end up with all of the capital, which is a kind of stability, but maybe not a desirable one. [Laughs.] I used to think that Japan would end up with all the world's capital because of their high savings rates, especially if you were to complete the markets more, so that we can borrow even more from them. Then we should become more and more indebted—in fact we should become indebted at a more and more rapid rate—as the Japanese end up not only owning all of our capital, but also the present value of our future labor productivity and future human capital productivity. Of course it didn't quite work out that way.

**Woodford:** That fate seems less of a threat now.

**Brock:** This points to the importance of institutions. I also did work on political economy and institutions, and you never want to forget that when you're doing mathematical economics. Government matters, the Fed matters, institutions matter, and all these things can work to support or counteract the forces for stability. They can generate a lot of instability, like through poorly managed banking regulations.

**Woodford:** What is your view of the importance for economics of the idea of chaotic dynamics? I know that you were interested for a while in studying chaotic systems.

**Brock:** That's one of those things where, in my heart of hearts, when I started looking at chaotic dynamics, being from Chicago and Rochester I didn't think that there was any chance of chaotic dynamics. But being a scientist, I felt that one should test, rather than just simply make an ideological statement about it.

So Dee Dechert, José [Scheinkman], and I got involved in this project of testing, and we cooked up statistical methods to test for chaos.<sup>8</sup> Well, it's not really a "test" of chaos, of course, but at any rate it's an indicator consistent with chaos and other "deep" nonlinearities of that kind. I would say the evidence for low-dimensional deterministic chaos in economic and financial data is pretty weak. I know that there are authors that have said the contrary; time will tell. Of course, the world has got to be high-dimensionally chaotic, in some philosophical sense. But you know, I don't think anybody will ever invent statistical methods that are precise enough, on machines with finite resolution and with finite data sets, to ferret out a high-dimensional, highly irregular chaos, as opposed to pure randomness. That's a tough problem.

**Woodford:** And it may not matter to us, even in principle, to know the difference between these two descriptions of the world.

**Brock:** That's right. I think the reason why people got so fascinated with this was that it was kind of like breaking a secret code or something. If, say, six-dimensional chaos is generating the data that you see, there is the potential of being able to reconstruct that chaos and forecast much better.

**Woodford:** Better, you mean, than with a linear statistical model.

**Brock:** That's right. I think that's inherently just a beautiful problem. I do have a little bit of the attitude of the mathematician; I think it was Andre Weil of Princeton, who said, "If it's beautiful, it must be useful." And this stuff is just so beautiful that a person has to play with it.

And I think one of the prettiest things that we got, besides the central limit theorem, was the following. Suppose you estimate a process of the form

$$y_t = f(\mathbf{x}_t, \boldsymbol{\theta}) + \varepsilon_t,$$

where  $y_t$  is observable data,  $\mathbf{x}_t$  is a vector of observables,  $\boldsymbol{\theta}$  is a vector of parameters, and  $\varepsilon_t$  is i.i.d. error. Then, as long as  $\boldsymbol{\theta}$  can be estimated  $\sqrt{n}$ -consistently, the first-order asymptotics of our statistic are invariant to estimation error. This usually doesn't happen, but there is a mathematical symmetry in this statistic that we constructed that worked to allow us to get this result. This is pretty.

And even though the evidence for chaos turned out to be pretty minimal, in my opinion, the statistic itself has been used by all kinds of people for all different kinds of tasks. Everything from specification testing of GARCH models to doing specification testing in general, or just testing for nonforecastable nonstationarities that may not have been recognized, or testing for nonlinearities or other structure that has been left out. It's used for nonlinear models that people fit in data, and people have adapted it to cross sections as well as time series. So I think it's one of those things that ended up producing a useful tool, whether it had anything to do with chaos or not. It turns out that our test has high power against chaos, but it has high power against a lot of other stuff too.

**Woodford:** Setting aside the issue of deterministic chaos, do you think that the search for nonlinear structure in economic systems is important? Or would your conjecture tend to be that linear approximations probably describe most of what's important about the dynamics of economic systems?

**Brock:** Well, that is a tough one too. It is very hard to beat a linear process out of sample, in many contexts in economics. If you condition on the right statistics, like volume movements and past volatility movements, people like [Blake] LeBaron have shown that you can actually beat a linear model or a random-walk model or a simple martingale-difference sequence model in finance. But, in general, it is hard to beat linear models out of sample.

I think part of the reason for that is the noisiness of the data in macro, especially if you don't have enough observations or the data are not disaggregated enough. I have seen studies where if you go to lower levels of aggregation, you will find asymmetric movements across the business cycle that are not consistent with a vector autoregression model. If I have a VAR system of the form

$$x_{t+1} = Ax_t + \varepsilon_{t+1},$$

and if the errors  $\varepsilon_{t+1}$  are symmetrically distributed, then asymmetries in the output series  $x_t$  are not consistent with such a model. People have detected a fair amount of evidence for asymmetries, like unemployment rates that tend to shoot up fast during a recession and dribble down slowly. I have seen stuff like that. I have also seen things that document other business-cycle asymmetries if you go down to lower levels of aggregation. I don't know how good the evidence is for that, but I think it is important to document whether they are there or not. It could have an impact on how we view the economy.

It is certainly known that recessions are shorter than expansions: People have shown that post-WWII expansions are roughly 4 times longer than recessions, and the duration of an expansion when you condition on its length seems to be independent of its length. This kind of nonlinear thinking sort of propels you into asking questions about asymmetries, which I think is healthy for the profession, especially in macro, which I think is sometimes a bit too log-linear for its own intellectual health.

You don't want to close off a line of inquiry prematurely. I think the profession can afford a few people experimenting. You know, it's very much like the multiarm

bandit problem, where if you are discounting the future, you can get stopped on the wrong machine, and it could happen with a very substantial probability, even though you are doing optimal learning. Whereas, if you don't discount the future—and I don't think that science should discount the future, or at least not very much—then you should always sample that other machine, even though it looks bad right now, on a sparse set of times. So to a certain extent, some of this exotic stuff in economic science, like chaos, should be studied for that reason.

I don't even think nonlinearity in general is that exotic, because if you look at the microlabor literature, those guys are completely nonlinear. They can't understand what the fuss is about. I don't think any microeconomist would even ask questions about nonlinearity; to them it is just a matter of common sense. But the macro people, I think, are working with data of less quality. One thing I have noticed, having worked in a lot of fields, is that the better the data quality in the field, the less argument there is about high-technology methods, such as testing for nonlinearities, or using neural networks or looking for chaos, all of that kind of stuff.

And in the financial field, which has to meet the bottom line, there wasn't any argument at all. They just simply took the stuff. I have lectured on Wall Street and found working papers sitting on the desk in the anteroom while waiting to go in to lecture. For these people, if there is any argument, they simply try it and if it works, it is part of the tool kit bag. And if it doesn't they throw it out; that's it.

**Woodford:** Do you think that they are interested because they see that it works, or because the high payoff in their field to having something before anyone else makes them willing to try out even exotic strategies?

**Brock:** I think they know what it is they are doing. Their goal functions are very precise. That might be a better way of putting it. In the case of finance it is profit, but even in that maligned institution, government—and I have worked in quite a few government sectors, not only here but in New Zealand and in Australia—when you are working for an arm of the government, like the Federal Trade Commission or the Department of Justice, you know exactly what it is you are to be doing, much more precisely than when you're trying to get a paper published in a journal like *Econometrica*. You know that they want you to know how the system works, because if they conduct the policy and it doesn't work, and if what you told them turned out to be wrong, they are liable to be left twisting in the wind. So there is a strong incentive to understand how the system works, even if they might be politically opposed to what you are telling them. Because that will just give them a better set of arguments to advance their political position, so even there I felt I had a much better idea what I was trying to do. My job was to understand the system the best I could, and report accordingly.

Things are different in science. Durlauf and I wrote a humorous little paper<sup>9</sup> on the economics of science, where we had scientists getting paid off if their theories predict better out of sample than rivals', but also being punished if they deviated from their reference group. And we worked this out using a little social-interaction model that we were working on. You can get alternative stable states in these kinds

of models, where, in order to survive you have to affiliate yourself with a school as it were, and if you deviate from that school you get punished.

**Woodford:** You think that this is more of a problem in science than it is in business or government?

**Brock:** I think so. Because in business you have to meet the bottom line, period, or you are going to be put out of business by someone who does. And in government, you have to understand the system; if you tell people stuff that later turns out to be false, your reputation goes through the halls of the world's governments as being incompetent, and then you are out of business as a government consultant. I'm stating the thing starkly; of course, the real world is not that stark. But I think the direction of what I just said was right relative to academia, where the force of performance-based evaluation on a concrete performance metric isn't so strong.

**Woodford:** You have recently gotten interested in modeling bounded rationality.

**Brock:** Yes, see, there again for the outsider it looks as though this guy is running off on tangents again, but let's go back to the beginning. I got fascinated with stability, and developed a bunch of mathematical models and instruments to help me understand the forces of stability, and I got a pretty good grip on that. Then I wanted to know what forces caused instability; they certainly looked like they could be there: incomplete markets, liquidity constraints, institutional breakdowns, et cetera. So then what kind of evidence is there for stability, what kind of evidence is there for instability? It actually forces you to econometric methods, and in macro it's going to be time series.

And so then I got interested in testing methods for the most unstable form of instability you can think of, which is deterministic chaos, or noise-perturbed deterministic chaos, since nobody in economics, especially in finance, is nutty enough to believe that financial markets can be deterministic. Nobody believes that. But the issue is, could there be a noise-contaminated deterministic mechanism such that when you shut off the noise, it has enough nonlinearity in it to generate some fluctuations on its own. Intelligent people have asked questions like that. Even people like Keynes talked about things like that. So it's a legitimate question to ask. That got me interested in the work that I did with Dechert, Scheinkman and LeBaron. David Hsieh, [Blake] LeBaron, and I wrote a book for the MIT Press,<sup>10</sup> where we tried to set this type of econometric method in with the broad field of time-series econometrics.

And so, in searching for evidence of instability, the natural thing to do is to look to see if there is left-out structure in the residuals of standard models, such as a VAR in the case of macro data. And then we ended up defining notions of linearity. That turned out to be a nontrivial technical problem in the stochastic setting, because every stationary process has a Wold representation where the errors are uncorrelated. So we defined notions of linearity, like "i.i.d. linearity," that you can test—assuming regularity conditions, of course. A series is i.i.d. linear if it has a Wold representation with i.i.d. errors. But that's too strong a notion for finance, because everyone knows about heteroskedasticity in finance. So we cooked up a notion that we called "MDS linearity," and that's a Wold

representation with martingale difference sequence errors. The BDS statistic that Dechert, Scheinkman, and I cooked up turns out to be ideal for testing for i.i.d. linearity. But that is too strong a null hypothesis for finance. But then for MDS linearity, you can parameterize the martingale difference sequence—the GARCH literature is one way of doing that—and have ultimate i.i.d. drivers. So you can estimate the parameterized form of the MDS, suck out estimates of the ultimate i.i.d. drivers, and test *them* for independence. It turns out that you can adapt the BDS method for doing that.

That work was asking, is there anything else in economic data besides the so-called received theory, which are more or less variations on linearist methods: a Wold representation with a type of nonlinearity in the martingale difference sequence, but that still looks very linear. You could think of it as linear in conditional volatility rather than linear in the conditional mean. It was running into statistical problems carrying out that program that drove Lakonishok, Lebaron, and me to invent what we think is a more sophisticated method for testing for departures from conventional models in time-series data. That was our *Journal of Finance* paper in December 1992,<sup>11</sup> which got used a lot, as I understand, by practitioners.

The idea there is that you take any trading method used by practitioners. We focused on technical trading rules, but it does not have to be; it could be fundamentalist trading rules. You take the rule that looks like it might be of use to practitioners, and you take any received “law” that is strongly defended by a school of economics, like in the Brock and Durlauf paper, by a powerful school. So we took a GARCH model as our received model that is going to be the null hypothesis. Estimate the received model on data; then bootstrap trading-rule performance statistics. So on each pass through the bootstrap, you buy when the rule says to buy, then you hold for so many periods, close out the position, and compute the conditional mean and the conditional variance following the buy signal. This gives you two statistics. Compute the conditional mean and conditional variance following the sell signal. This gives you two other statistics. Then compute the profits from this trading rule, giving you five statistics altogether. You can essentially form a multidimensional histogram of these five statistics.

Recompute the same five statistics for your data, and tick off a rejection region or report a  $p$  value. I prefer to report  $p$  values under the null, and let the readers make up their own minds whether the evidence is compelling or not. If you get a small  $p$  value of the null, reject the null hypothesis. Now the benefit of this procedure over what I’ll call conventional econometrics—maximum likelihood estimation, GMM, extremal estimators, and the like—is that those methods are not really tailor-made to the application in question. The statistics ground out by those methods have nothing to do with the objectives of the players.

**Woodford:** In a sense, you want to test the rational expectations model by asking if people should learn to have those expectations.

**Brock:** Yes. I like the self-referential or self-consistent feature of this approach, that the statistics were directly related to the objectives of the players that we were trying to model, and that the conventional model purports to model. And then the



bootstrap is available, so why not use it? Analytically, asymptotics for these kinds of statistics are beyond reach, but who cares? Under Moore's law, with the rapidly declining cost of computation, ....

**Woodford:** You may as well do Monte Carlo studies.

**Brock:** Yes. So you use the bootstrap, and then there is a rigorous statistical theory worked out for the bootstrap. It has its own central-limit-type results for consistency under the null and so on. Now we couldn't use any of those results after doing extensive literature research, but you can use the computer instead. What you do is generate a sample of say 100,000 observations from your favorite null model. Then you pretend that you are these three statisticians, Brock, Lakonishok, and LeBaron, going through their procedure for a sample of length 1,000, 2,000, 3,000 under the null, and see if their five bootstrap histograms converge, by the size of sample length that they actually have. While our available sample length was 2,000 for all of the subsets of data we looked at, the thing appeared to converge around 500 or so. So that is a kind of "computer" proof of consistency, that it was *usably* consistent.

**Woodford:** So you show not just asymptotic convergence, but even with the length of sample that you have, which is a more usable result.

**Brock:** There are a lot of statistics used out there that converge so slowly, even though econometricians have proved asymptotic theorems, that are useless. So I see all of this as part of an applied project of trying to understand patterns that I see in the economy, which may be driven by stabilizing forces or destabilizing forces that may or may not be captured in the profession's received models. We invented these methods to look for departures from those received models. And then that drove me to look at bounded rationality, because we saw so many departures in the financial area. And trading volume seemed way too huge to be consistent with everybody having the same expectations and common knowledge, given the no-trade theorems, et cetera. So I wanted to understand what is causing the departures from the received theory, that I know and love and contributed to as well.

**Woodford:** And your working hypothesis about the nature of the more adequate theory is that it would involve departures from rational expectations?

**Brock:** That's a tough question. I can tell you how we're approaching it. We developed a general theory with Darwinistic selection-type forces over a space of expectational types, and rational expectations is just one of the types. And then within this general theory, you can work out competitive equilibrium, and get restrictions on the data, which you can test using the generalized instrumental variables literature, Hansen-Singleton *et alia*, that were invented in the context of testing rational expectations models. We have been able to carry out the econometric part of that program, in the sense of generating a theoretical framework so that we can copy a lot of these econometric techniques.

A student of mine, Saangjoon Baak, has carried out one paper<sup>12</sup> which extends the Rosen, Murphy, and Scheinkman cattle-cycle paper, that assumed rational expectations. Baak set up a general model with two kinds of ranchers: Rosen-Murphy-Scheinkman ranchers and backward-looking ranchers. It was linear-quadratic, so

you could solve for equilibrium, and then he applied some of the technology of Anderson, Hansen, Sargent, and McGrattan.

**Woodford:** This is their chapter in the *Handbook of Computational Economics*?<sup>13</sup>

**Brock:** That's right. So that was the perfect framework for him to use, and he tested for the significance of the extra parameters resulting from the addition of the boundedly rational ranchers to the model. Within that context, he accepted the hypothesis that the boundedly rational ranchers were there, and something like 25% or 30% of them were boundedly rational. Then he looked at another type of backward-looking expectations that is more rational. It is called "quasi-rational expectations," and it is due to Marc Nerlove. The idea is that you form expectations that are consistent with past autocorrelation functions, but you don't assume that the rancher knows the structure of the world he is living in. So this is a halfway house. As you might expect, the likelihood function liked the Nerlove hypothesis better than a stupid backward-looking boundedly rational rancher. About 70% or so were estimated to be Nerlovians in that version of the model.

**Woodford:** Although it was still important, even so, to allow a part of the population to have rational expectations.

**Brock:** Exactly, even in the Nerlove case, it looked like about 30% or so. And then a colleague of mine, Jean-Paul Chavas, who teaches in the agricultural economics department here, has a different technique, more of a GMM-type technique, but addressed himself to the same problem. I think that he might have looked at cattle too, though I'm not so sure about the cattle, but I know he looked at pork.<sup>14</sup> And he found a roughly similar percentage of boundedly rational agents. I think he looked at Nerlovians too, so it's encouraging that the two parties used different methods and got rough agreement, though like in most kinds of econometric results, it is really rough. Anyone who does empirical work will be sympathetic to what I am talking about here, how you have to take each result with extreme caution. But I think the general strategy is sound, of laying out a framework that includes versions of rational expectations nested within the framework, and doing the theory in such a way that you can use received econometrics, generalized instrumental variables and the like, and then let the data speak to whether the expectations are rational or not, rather than imposing it.

**Woodford:** This sounds to a large extent like a continuation of the kind of research that was being done prior to the advent of rational expectations, notably by Nerlove but also by others, where there were attempts to econometrically estimate models that included explicit models of the expectations, in terms of some kind of backward-looking expectations.

**Brock:** That's right, those guys would look at things like distributed lags, and that was very popular.

**Woodford:** What you're doing is of course a more sophisticated version of those specifications, but it would seem to be in the same spirit.

**Brock:** I think that it has a somewhat similar spirit, in the sense that rational expectations didn't really exist when those guys were working. That was invented later, and then econometric methods were developed to do inference and testing



**FIGURE 3.** Tap dancing at the Economics Department holiday party, University of Wisconsin—Madison, December 1998. Photo by Niemke Oomes.

in the framework of rational expectations. Now at last we have enough tools out there and enough theory developed, where I think we can now develop a framework where we let the data speak to whether the expectations are rational or not. You still have a lot of tough econometric compromises that you have to make in order to carry out such a program, like assuming stationarity. I know people who would rather commit suicide than assume stationarity, and there are issues of detrending, whether you work in a difference-stationary framework or a trend-stationary framework, et cetera.

**Woodford:** But those complications aren't special to the nature of your hypothesis.

**Brock:** That's right. We are carrying out a similar program in finance as well, and what is handy in finance is that volume data is very powerful in slicing across different classes of models. Because even in noisy rational expectations models, you get positive volume in those models, but you can ask about the pattern of reversion of volume to a long-run level, and how long it takes for it to revert to a long-run level. This can give you some idea of how much heterogeneity there

might be in expectations above and beyond signal heterogeneity in the context of noisy rational expectations models.

That's where I am now, working on this bounded rationality program. I'm working with a really excellent young scholar named Cars Hommes, at the University of Amsterdam, who just received an award from the Dutch government to set up a center where he could do experimental work as well. He has ideas on how to do laboratory experiments on expectations, as well as carry out our research program on what you might call general theories of expectations formation. We call it "evolutionary economic dynamics," where rational expectations plays a major role because it is nested within the general evolutionary theory of expectations formation.

**Woodford:** But the empirical strategy that you have described doesn't directly really model this evolutionary process though, does it? You only mentioned estimation of fractions of the population that form expectations in various ways.

**Brock:** Yes, it does. What we have is a discrete-choice model where each agent in the model solves a discrete-choice problem, as in the work of McFadden and Manski, or Rust, and Honoré, over a population of possible predictors or expectational trading strategies. How the mass of traders move across these expectational trading strategies depends on performance, so that as profits or losses roll into each strategy, the mass of traders moves across the space of such strategies. The "intensity of choice" in McFadden-Manski terminology determines how fast they move in response to profit differentials from different strategies.

**Woodford:** So the fraction of the population with different strategies is changing over a timescale that is short relative to your sample?

**Brock:** Exactly, and that makes executing an econometric inference strategy difficult. Because in the work that I mentioned by Baak and Chavas, what they basically did was to assume that there is just a fixed fraction in each type.

**Woodford:** So there is no evolution in those models.

**Brock:** And so they tested a null hypothesis that the fraction is 100% in rational expectations, against the alternative that there is a positive fraction who use some boundedly rational rule. But in the full endogenous theory that Hommes and I have developed—we have one paper in *Econometrica*,<sup>15</sup> another one in the *Journal of Economic Dynamics and Control*,<sup>16</sup> and there are a couple of others forthcoming—all of those have endogenous evolution of the probabilities across strategies over time. We explore sources of stability or instability in that context, and one of the important parameters is the sensitivity or intensity of choice. In other words, if you had a population of people that are highly sensitive to the slightest advantage, then that could even lead to instability, because all of the traders will sort of switch *en masse* from one strategy to another in response to a very small profit differential.

**Woodford:** And a small apparent differential of that kind could occur by accident.

**Brock:** That's right, and that would make the system lurch from one place to another. It's like they are all highly tuned—"Chicago"-style—to the tiniest difference in incentives; then the herd can just move *en masse*, rather like Samuelson's boat. You know the parable of Samuelson's boat?

**Woodford:** No.

**Brock:** I think this is from Paul Samuelson. A bunch of people are on a long linear boat, and they are whale watching. Somebody yells “Whale!” on the right of the boat, and they all run to the right side, and the boat starts to sink. They all panic and so all run to the left side. But now they are all on the left side, and the boat sinks even more. This system has an unstable eigenvalue in discrete time, going through  $-1$ .

You can get similar waves of movement across expectational strategies in our evolutionary models. Profits are all over here, then vroom! they all go over there. So this lurching back and forth between expectational types can be rather unstable.

**Woodford:** That’s a pretty alarming picture of the functioning of markets.

**Brock:** Of course, the instability only occurs if the intensity of choice is large enough. If the intensity of choice is low, then the people would act more randomly and be more uniformly distributed across the boat. Some would probably go to see the whale, but others would stay put, and then the system would not be destabilized.

I like this as a metaphor to make the point that if you have high intensity of choice across different expectational strategies, then when it looks like there is an advantage to using rational expectations, even after you pay the higher costs, then everybody would start using rational expectations. But then all the rent would disappear to rational expectations, and their costs would not be covered. And then everybody would try to get by using cheaper expectations and avoid those costs. This way you could generate instability with high enough intensity of choice.

**Woodford:** The parable seems to depend not just on a strong enough intensity of choice, a strong enough response to changes in forecasts, but also on a high degree of synchronization in people’s reactions. The time lag in the reaction has to be the same for this whole group of people.

**Brock:** That’s right. Replacing synchronous updating with asynchronous updating would dampen such a force. I actually think this problem infects much of economic dynamics, including the “sunspots” literature. Why would everyone synchronously lock onto the same sunspot, and why wouldn’t something like the law of large numbers squish such effects? I tried to deal with this problem in *Estudios Economicos*<sup>17</sup> by proposing a framework that joins tools from discrete-choice econometrics and statistical physics, where complementarities across agents are strong enough that the law of large numbers fails and “clumping,” rather like asynchronous updating, occurs. Then the force comes back again. It’s like a little bit of “Yale sociology economics” plus a lot of “Chicago sharp-response-to-incentives economics” leads to a breakdown of the law of large numbers, so you don’t get the “washout effect” that you were alluding to anymore.

Anyway, abstracting from that problem, it’s rather like the Grossman-Stiglitz information paradox, but dynamized. Those guys argued that if everybody was using rational expectations and they were costly to obtain, then it wouldn’t pay me to buy into rational expectations. I would use naïve expectations, especially if the system had settled down to some kind of stable state, because you don’t need sophisticated expectations to predict next period’s state of the system when

it is not changing every much. But if nobody used rational expectations, then in a situation where naïve expectations was locally unstable, like a supply-and-demand situation where the elasticity of supply is greater than the elasticity of demand—a hog-cycle situation, for example—you could get instability of the following type.

If everybody uses naïve expectations, say backward-looking expectations, then you get something like an unstable hog cycle. Then if that instability grew, it would be worth my while to buy into rational expectations, even though they are costly, because I can cover those costs with the extra profits I can make by more accurately forecasting those fluctuations. But then when those fluctuations died down, again it wouldn't be worth my while, and the whole story would repeat.

Note that you really need two forces to generate this kind of instability. One is that a steady state is locally unstable under naïve expectations, and on the other hand, the intensity of choice has to be high enough to get a “Samuelson's boat”-type phenomenon across expectational strategies.

**Woodford:** So greater sophistication, in the sense of responding rapidly to changes in the incentives to use more sophisticated kinds of forecasting, is actually a force for instability.

**Brock:** That's right. It could happen and that was what Hommes and I showed in our *Econometrica* article.

**Woodford:** Is this something that you think is more likely to be a serious problem as financial market participants become more sophisticated?

**Brock:** It could be. A student of mine, Patrick de Fontnouvelle, re-did the stuff that Hommes and I did in the context of “noisy rational expectations” models where it is possible to get more precise signals.<sup>18</sup> If everybody buys into more precise signals, then there is not enough rent to cover the cost of purchasing more precise signals. So more of the traders purchase coarse signals, and then, even though you get a martingale difference sequence for returns, it is more variable because people are not purchasing as accurate a signal. So you get phases. A de Fontnouvelle system runs through these phases where signals are quite tight for a while, and then they are quite loose, and then they are quite tight again. The logic of it is very much like a conjunction of the logic of my paper with Hommes and the old Grossman-Stiglitz information paradox.

**Woodford:** Is it your view that this kind of instability in the way people are forming expectations is something one should be able to see occurring over relatively short timescales, that in actual data that we would see the dynamics unfolding?

**Brock:** I think so. We've got a project where we are trying to use data on what fraction of stock market investors are in index funds. Our idea is to study volatility and transactions volume, and what the implications would be if the entire market was in index funds and didn't do security analysis at all. My conjecture is that observed variations in volatility and volume may be due to changes in the number of traders using different types of expectational strategies. But that hasn't gotten very far yet.

**Woodford:** Your perspective seems to be quite different from much recent work on bounded rationality. Many people propose to use models of bounded rationality

as a way of selecting among equilibria, still assuming that the rational expectations equilibria (or Bayesian Nash equilibria) are the things that one would actually see. An equilibrium would still be the prediction of the model, once one selected the right one.

**Brock:** Yes, I am much more interested in the kind of logical inconsistency embodied in the Grossman-Stiglitz paradox, and how it might show up in actual data, as people oscillate between patterns of behavior that are each self-contradictory, rather than ever reaching an equilibrium at all.

We have a project where my co-authors and I plan to test the implications of those kinds of theories on volume and volatility data. The idea is very much like the original rational expectations literature, where you get orthogonality restrictions imposed by full rational expectations, and then you test those restrictions using generalized instrumental variables methods, like Hansen and Singleton did. What we do is nest the conventional rational expectations theory in this larger structure where evolutive pressures unfold over time, and then set up a nested econometric framework to test whether the extra parameters, indicating the presence of the boundedly rational players or the coarser-signal players, are significant.

**Woodford:** Have you talked to actual traders in financial markets at all about these ideas?

**Brock:** Well, [Allan] Kleidon and I wrote a paper<sup>19</sup> on bid-ask spreads at the open and close of trading, and he actually spent time on the floor of the New York Stock Exchange in one of their visitors' programs. And I have also discussed this kind of thing in lectures to financial types. And some of my associates that have been here are actually working for trading houses now. I don't think I should identify the firms, but I think the ideas are being explored.

**Woodford:** And do they find that this idea of dynamics in the way people try to get information and try to forecast the market is actually realistic? Is it helpful in understanding things that they see happening?

**Brock:** Well, so much of their work is proprietary. But I guess all of these guys are highly competitive. I know that some firms talk like they believe the markets are highly heterogeneous, and they believe there are hierarchies of expectation formations at different timescales, and that this generates something like scaling laws. Some of them got very interested in the work that Lakonishok, LeBaron, and I did, that we talked about earlier. This method can allow them to detect systematically departures from a null model, which could be their own trading model, and then repair it so that it predicts out of sample better.

**Woodford:** Is there a problem of internal inconsistency of your model that comes from the fact that if people understand it, then that will in fact undermine the validity of the model itself?

**Brock:** Well, I think in the signal version of the model, the version with noisy rational expectations, it is harder to undermine it. It is always tricky, though, when you get into these common-knowledge situations. I have never been able to completely solve the Townsend-type problem of expectations of expectations, to keep everything consistent in a common-knowledge framework. It was partly in



response to the difficulty of dealing with that that we went toward a more evolutive framework.

This is rather like the evolutionary approach that caught on in game theory, in order to get out of the self-contradictions that you inevitably roll up against in a  $k$ th order common-knowledge framework where  $k$  tends to infinity. I know that sounds awfully nerdy [laughs]. But you get caught in this anytime that you depart from full rationality; that type of contradiction will appear. We tried to escape by imposing costs to obtaining fully rational expectations, and then working out a dynamic version of the Grossman-Stiglitz paradox.

**Woodford:** You were involved early on with the economics program of the Santa Fe Institute.

**Brock:** That's right. I wasn't there at the birth, but I was there a year later, when we had the big conference out of which the Anderson, Arrow, and Pines volume<sup>20</sup> came.

**Woodford:** Did that experience affect the development of your own ideas much?

**Brock:** Yes, I think so. Because I was kind of being driven in that direction by doing this empirical work. I was already working with people like Allan Kleidon, who is very empirically oriented in finance, and Blake LeBaron and Joseph Lakonishok who are also very empirically oriented. And forcing yourself to confront these patterns in data doesn't necessarily make you think like the Santa Fe Institute, but I think it kind of does make you less ideological about imposing a really high degree of rationality, like  $k$ th order common knowledge where  $k$  is huge.

**Woodford:** Do you think that economists have much to gain from interdisciplinary efforts—from trying to work on economic problems with physicists or biologists or computer scientists, for example?

**Brock:** I think I have learned a lot out of it. You have to apply a lot of judgment, so that you don't go running off on some nonproductive fad. There is a tendency for those guys to write down backward-looking models, whereas the logic of economics forces you to write down forward-looking models where the agents have intelligence enough to understand the system they live in, at least partially. Those guys are not used to modeling that way, and there tends to be a conflict there. And there is also kind of a tendency to speak out on economics when they are not all that well informed, which Paul Krugman has written very well about.

**Woodford:** Which way do you think studies of economic dynamics will develop over the next 25 or 50 years? Do you think that we are on the verge of any important change in direction?

**Brock:** I think it is going to look more like a conjunction of numerical and computational methods of the kind treated in Judd's book,<sup>21</sup> and computational statistical inference like the work on the bootstrap. Because as the cost of computational inference drops, we will rely less on analytic fiddling around in order to handle complicated inferential problems. We'll fiddle a lot less with analytic asymptotic theory in econometrics, and rely much more on computational inferential tools

like the bootstrap. But we'll be working with more realistic models, more along the line of Judd. I think that this is going to happen as computational technology keeps dropping in price, at least if it keeps dropping exponentially like it has in the past.

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